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Dear Josh,

First of all, very many congratulations. We were all delighted, Lorna and myself, also all our family. And we don't think it will be too long before you get another prize, perhaps for something to do with somatic cells the next time. Marvelous.

Then, I've been looking through the information about Stanford. It all looks just too good to be true. But no less than you deserve. A new building by Edward Stone; the best biochemist as a colleague; other colleagues who are vitally concerned with the same sort of things as yourself, especially Kaplan and Goldstein. Of course what made me most delighted in the whole set-up was your own proposals for research: obviously there's going to be a lot more coming out about epinuclear genetics, and that's the main thing now. We spent so much time at Carradale talking about antibodies that we never properly got round to the epinuclear future in microorganisms. My own guess is that this is where the most exciting things are going to emerge in biology in the immediate future - they'll come sooner than, say, sex in somatic cells. And it's up to you to find them.

You know how honored I felt by your offer to me. And the terms were magnificent: plenty of research, some teaching, and more dollars than I can imagine ever being able to spend. But the main thing was - and I am being perfectly honest - that I would have been working with someone really exciting who really wanted done what I could hope to do. What I said in the Summer was the truth. There's nothing I'd rather do as a scientist. But there's more to life than that.

Next, what fun it will be for us all to meet again next June at the Ciba meeting in Paris. I see that you've left your title open. I hope that you'll have something more to say about tolerance. I'm sending you (by surface mail) a copy of my draft for an earlier meeting, to be held in March in Liege. You will see that I've tried to give an account of your hypothesis - I hope it meets with satisfaction.

I think ^{my experiment} it shows that the presence of antigen is definitely required for the maintenance of tolerance to erythrocytes. ~~Together with~~ It fully confirms the important paper of Smith and Bridges which came out a few months ago. You will see that I have speculated about a possible test of your hypothesis, according to variance in the time-course of the loss of tolerance. But I don't feel particularly optimistic about this.

Is there any chance of yourself and Esther spending a few days on the Riviera before the meeting, with Cavalli and Donald Michie? Donald is an old friend of mine, and also of Cavalli. He's going to Italy under Cavalli's auspices in May, and we thought it would be great fun if we could all spend a few days on a Mediterranean beach together. He has asked Cavalli, but has not heard from him yet (at least as far as I know). Would this be the best time for you, or would you prefer after the Ciba meeting? Do try to come.

Finally, may I introduce a friend at long range? Dr J.L. Gowans will be making a journey through the USA next January and February. He is extremely keen to meet you - indeed you may have heard from him already. I do not think your time would be wasted if he calls in. He has just completed a most important experiment on the lymphocyte - that much misjudged cell. So far from living only a few days, the lymphocyte appears to live for many weeks, continually recirculating from the blood to the lymph nodes. This may have implications for antibody production (though I think Jim has doubts whether the small lymphocyte at least has anything to do with immunity). He comes from the Pathology department at Oxford, Florey's lab.

Yours ever,
Arthur